DOCUMENT RESUME

ED 113 647 95 CG 010 151

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TITLE	Needed Research on PEM Aspects of Child Development. A Special Report of the USOE-Sponsored Grant Study: Critical Appraisal of Research in the Personality-Emotions-Motivation Domain. IBR Report No. 73-25.
INSTITUTION	Texas Christian Univ., Fort Worth. Inst. of Behavioral Research.
SPONS AGENCY	Office of Education (DHEW), Washington, D.C.
REPORT NO	IBR-R-73-25
PUB DATE	Oct 73
GRANT	OEG-0-70-2665(508)
NOTE	93p.
EDRS PRICE	MF-$0.76 HC-$4.43 Plus Postage
DESCRIPTORS	Behavioral Science Research; *Child Development; Disadvantaged Environment; Human Development; *Learning Processes; *Personality Development; *Research Needs; Serial Learning; *Social Development; State of the Art Reviews

ABSTRACT

The task group report presented in this publication is one of a series prepared by eminent psychologists who have served as consultants in the U.S.O.E.-sponsored grant study to conduct a Critical Appraisal of the Personality-Emotions-Motivation Domain. In order to achieve the goal of identifying important problems and areas for new research and methodological issues related to them, an approach was followed in which leading investigators in specialized areas were enlisted as members of task groups and asked to reflect on their current knowledge of ongoing research and to identify the research needs in their respective areas. William Meyer has written an introduction to this publication. Topics covered are: (1) A Discussion of Needed Research Including Theoretical and Methodological Considerations in the Area of Personality-Emotions-Motivation with Emphasis on Forced Training, Readiness, and the Sequential Organization of Curricula (Hall); (2) Infancy and Early Childhood: Research Needs in the Study of Socioemotional Development (Lewis); (3) The Status of Research of Social-Emotional Development (Sigel); and (4) Ecology and Development: Future Directions (Meyer). (Author/HMV)

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FOREWORD

The task group report presented in the following pages is one of a series prepared by eminent psychologists who have served as consultants in the U. S. Office of Education sponsored grant study to conduct a Critical Appraisal of the Personality-Emotions-Motivation Domain. The study was planned with the advice of an advisory committee including Professors Raymond B. Cattell and J. McV. Hunt (University of Illinois), Donald W. MacKinnon (University of California, Berkeley), Warren T. Norman (University of Michigan), and Dr. Robert H. Beezer (USOE) and follows a topical outline included as an appendix to the present report. In order to achieve the goal of identifying important problems and areas for new research and methodological issues related to them, an approach was followed in which leading investigators in specialized areas were enlisted as members of task groups and asked to reflect on their current knowledge of ongoing research and to identify the research needs in their respective areas. The general plan is to publish these reports as a collection with integration contributed by the editors. It is hoped that these reports will prove to be valuable to research scientists and administrators.

S. B. Sells, Ph.D.
Responsible Investigator

FOREWORD

The task group report presented in the following pages is one of a series prepared by eminent psychologists who have served as consultants in the U. S. Office of Education sponsored grant study to conduct a Critical Appraisal of the Personality-Emotions-Motivation Domain. The study was planned with the advice of an advisory committee including Professors Raymond B. Cattell and J. McV. Hunt (University of Illinois), Donald W. MacKinnon (University of California, Berkeley), Warren T. Norman (University of Michigan), and Dr. Robert H. Beezer (USOE) and follows a topical outline included as an appendix to the present report. In order to achieve the goal of identifying important problems and areas for new research and methodological issues related to them, an approach was followed in which leading investigators in specialized areas were enlisted as members of task groups and asked to reflect on their current knowledge of ongoing research and to identify the research needs in their respective areas. The general plan is to publish these reports as a collection with integration contributed by the editors. It is hoped that these reports will prove to be valuable to research scientists and administrators.

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1000 - Personality-Emotional-Motivational Aspects of Child Development

Task Group Chairman
William J. Meyer
Syracuse University

The preceding decade has witnessed an unprecedented outpouring of work in the field of child development. This work was stimulated by the funding of programs for preschool age children from lower socioeconomic circumstances, popularly known as Project Head Start. As so often happens in education, Head Start, and other similar programs, were launched without a firm knowledge base with the consequence that, at best, programs reflected a state of general confusion on how best to proceed. Many programs, especially those sponsored by "old guard" early childhood educators, emphasized social and emotional development but without specifying in sufficiently clear terms what they meant by these terms. Furthermore, advocates of this view never made clear how a child could maintain an adequate "self concept" when confronted with academic failure nor did they specify how learning might eventually occur. The brash newcomers to the field completely emphasized learning; that is, their single mission was to provide the target population with the skills necessary for success in school. The social and emotional components of development, they argued, would accrue as a function of skill acquisition. But this strategy employed techniques that potentially, at least, would expose the child to failure

at a much earlier period in his life than might have occurred in the ordinary course of events.

The point of the foregoing brief and somewhat overstated introduction is that the programs developed for young children followed the traditional textbook sections; the separation of cognitive and affective aspects of behavior. Although such separation is necessary for book writers, it hardly makes sense in dealing with children. The four scientists contributing to this report clearly recognized this problem. Despite the emphasis on personality-emotional-motivational variables, each author found it necessary to integrate these rubrics with cognitive variables. I find this integration pleasing because they each warn, albeit implicitly, that it would be folly to pursue a research program focussing only on PEM -- perhaps this is the single most important contribution of this series of papers.

It will not be surprising to find that each author finds a need for theoretical models that integrate cognitive and affective aspects of development and for the development of measures consistent with these models. They also believe that longitudinal studies are required to answer the complex developmental questions each defines as important. And, finally, they all view the univariate design as inadequate to the task. The problems posed in this report are genuinely important and complex. They will not be resolved by making believe they do not exist and their solutions will be expensive.

I. A Discussion of Needed Research Including Theoretical and Methodological Considerations in the Area of Personality-Emotions-Motivation with Emphasis on Forced Training, Readiness, and the Sequential Organization of Curricula

Vernon C. Hall
Syracuse University

It is interesting to note that recently there has been a noticeable change in the interests of developmental-educational interventionists from major emphasis on intellectual abilities to more general types of behaviors categorized in this paper under the label of personality-emotions-motivation. This change is evident not only in recent issues of developmental journals but also in the priorities defined by the Offices of Education and Child Development (e.g., see Priorities from OCD for Office of Developmental Research and Demonstrations in Social Ecology Problems: FY 1973).

This change has occurred as a result of at least two realizations on the part of interventionists. First, there has been a gradual recognition that the predicted permanent changes in intellectual abilities (e.g., Hunt, 1961) resulting from early intervention were not forthcoming for the "culturally deprived" child. In spite of the massive amount of time and effort spent on many imaginative and unique early learning projects, the evidence for long term intellectual gain is infrequent and not particularly impressive. While for some this has meant a redoubling of efforts and adding an even bigger dose of intervention (e.g., suggesting that if the entire home environment is

changed then permanent changes will occur) others have recognized that we do not have the ability to speed up intellectual development and, in fact, there may be some unfortunate side effects in important areas outside of intellectual development. This may be particularly true for those who experience educational intervention without success or relatively short-lived unimportant gains. Although the evidence for harmful side effects is rather sparse they have been hypothesized by maturational developmental psychologists for some time. Second, psychologists have begun to accept the idea that even if intervention was successful and everyone could acquire an I.Q. of 125, we would still have not solved the problems that intervention was designed to solve and may have even created others. The world has had enough hostile geniuses. For many of the problem areas in our society which need solutions (e.g., how to successfully socialize children so that they will get along with other children and later with other adults or adjust to the rapidly changing societal conditions) more than academic intelligence is required.

Two specific and complementary areas which need research with regard to forced training, readiness, and the sequential organization of the curriculum can be identified from the above discussion. The first involves the effects of all types of academic educational intervention (both structured and unstructured) on nonacademic areas of behavior. The focus here is on both the child who shows rapid gains and the child who does not. Comparisons should also be made among children who have

experienced different kinds of intervention that reflect different philosophical positions. In addition, further attempts should be made (several have already been unsuccessful) to determine the nature of quantifiable child-characteristic by intervention-type interactions. The second area involves identifying the kinds of personality-emotions-motivation behaviors (or traits, depending on the researcher's preference) which can and/or should be taught. That is, is it possible to identify and teach desirable, relatively permanent behavioral dispositions? It is clear that this is a very sensitive area which needs to be approached with extreme care. While most Americans would agree that it is desirable to be smart and little outcry was heard against federal money being spent to make children smarter, this same kind of acceptance might not occur if the goal was to teach everyone to have high achievement motivation. Nevertheless I believe that the more information we have about the antecedents to relatively permanent behavior dispositions the better we will be able to make intelligent decisions about school environments. Even though such information could be used in dangerous ways, I believe that knowledge is always preferable to ignorance. Recently it was reported in the New York Times (September 16, 1972) that an organization called Neurotics Anonymous listed character traits which they believed create illness and health. Even though it represents a naive and rather inappropriate approach to the problem (i.e., assuming that character traits create or are antecedents to mental illness or health) it does

suggest that character traits can be categorized into desirable and undesirable categories.

The plan for the remainder of the present paper is to first become more specific about questions which need answering in the above areas and then to discuss the theoretical and methodological problems involved in such investigations.

Effects of the Educational Intervention Upon the Personality-Emotions-Motivation Domain

It is interesting to note that several of the efforts directed toward evaluating intervention programs during the sixties did include some measures other than those obtained on intelligence tests. However, fewer experimental-control differences were found on these measures than were found on those measuring "intelligence." Klaus and Gray (1968) compared their experimental and control groups on measures of conceptual tempo, self concept, reputation among peers, delay of gratification and achievement motivation. The only significant difference found was on the conceptual tempo measure where the experimental groups were significantly more reflective than the control groups.

The Westinghouse (1969) evaluation of Head Start included the Children's Self Concept Index (CSCI), the Children's Attitudinal Range Indicator (CARI) and the Classroom Behavior Inventory (CBI). There were no significant differences between experimental and control groups on any of these affective measures when scores of all subjects in the full year programs were used. Several comparisons were then made using experimental and

control groups from different geographic regions. The experimental subjects scored significantly better on two comparisons while the control subjects scored significently better on two others.

This failure to find differences may be due, at least in part, to the same reason that there were so few successful demonstrations of intellectual gains resulting from intervention; weaknesses in the evaluation process. There was a consistent failure to use evaluation instruments that were designed to measure the specific effects which the intervention was expected to produce. Instead standardized measures of general intelligence were used to evaluate all types of intervention even though some intervention programs were designed to effect gains in other areas. For instance, Weikart reports a study in which three groups of children who had experienced different types of intervention (curricula patterned after Piaget, Bereiter-Engelmann and a verbal bombardment program created at Ypsilanti) were compared. As an evaluation instrument all groups were given the Stanford-Binet. Because of no difference among these groups on this instrument, he concluded:

"The arguments about the relative effectiveness of various approaches to preschool education are irrelevant" (Weikart, 1968, p. 14).

I would doubt that Piagetians would be impressed or believe that it was particularly meaningful that subjects experiencing the kinds of intervention they recommend obtain large gains on

the Stanford-Binet (e.g., see Kohlberg, 1968). There are, however, a number of important changes which should take place and could be identified if the correct behaviors were measured. In other words, there needs to be a carefully articulated conceptual link between the philosophical underpinnings of the intervention and the evaluation instruments used. To date I know of no sets of carefully articulated goals for intervention programs which include detailed accounts of the personality-emotions-motivation domain. From detailed accounts of this kind predictions should not be difficult to derive for carefully planned intervention programs. With regard to specific programs now being used I am certain there are many hypotheses which need to be tested. For example, (1) are the graduates of a Montessori school more self reliant in new situations? (2) Do children who experience the open classroom get along better with their classmates than children experiencing other interventions?

With regard to the few hypotheses which have already been spelled out most have yet to be empirically tested or have not met with success. For instance, it is often hypothesized that an increased number (or higher percentage) of success experiences in school will lead to a higher, more healthy self concept. There have been few studies, however, which have systematically varied success experiences with a subsequent improvement in the child's self concept. One might argue that there may be serious limitations on the areas in which this success should be met if the measures of self concept are to change positively. For instance,

if these success experiences lead to an inaccurate self concept in a specific area or are inconsistent with experiences elsewhere, (i.e., outside of school) the child may reject the school's input or undergo unfortunate experiences when he learns that his perception of his abilities are inaccurate. There are two kinds of information that a child can have about himself, (1) how well he can perform in a particular area and (2) how well his performance in that area ranks with the performance of the rest of the children in his world. The child with a healthy adaptive self concept may need to know and accept both. If the child is given inaccurate information negative consequences could very well occur.

Another general belief is that pushing or teaching a child before he is ready will result in negative side effects in the personality-emotions-motivation domain, such as anxiety, dislike for school, low self concept, etc. The evidence for this kind of effect is rather sparse and needs careful empirical validation. This would include a careful specification of the conditions under which such reactions would be expected to occur.

Along similar lines of reasoning, nearly all intervention projects seem to assume that their procedures will be of equal benefit for all participants. I know of no intervention project which has attempted to observe or predict an interaction between student characteristics and type of intervention. I suspect that many important individual differences among children which could be of value to the teacher have yet to be identified. Preliminary

kinds of hypotheses along these lines would ask (1) do impulsive children become even more impulsive in an open classroom environment? (2) do children with high achievement motivation dislike contingency classrooms? (Atkinsen, 1965, has done some work along these lines with heterogeneous and homogeneous grouping) (3) when children are given choices among classrooms varying in degree of structure, what kinds of children choose which kind of classroom? Does this self selection seem more adaptive for some children than for others? (4) do children with certain identifiable experiential antecedents (e.g., pushing parents) fare better emotionally and/or intellectually than other children in a structured environment? (5) are lower-class children happier and more motivated in particular types of intervention environments than middle-class children?

Another area which has been neglected is concerned with the personality of the teacher. While some efforts are made to randomly select students for intervention, only those teachers who are willing to use the intervention prescribed are used. Since interventions are seldom monitored to determine what actually occurs in the classroom (and even if they were, there would still be ample room for the teacher to improvise) it is a good guess that the teacher uses many non-specified behaviors. It seems that a perfectly plausible area of investigation would be to determine whether teachers who teach in particular programs also share behavior patterns or personality dispositions. In

addition, it would be important to study the interaction between teacher characteristics and student change.

A final need is to determine what kinds of problems occur if early educational intervention is successful but primary school remains unchanged. In some middle-class areas the grade school teachers suggest that it may be better if children are not sent to preschools because then they are bored in kindergarten. With the rapid increase in day care centers and acceptance of working mothers, it becomes extremely important that effects in the affective domain be studied in terms of the transition from the preschool or day care center to grade school. If there are differences among children coming from different preschool programs then it is important that public schools become aware of these differences and adjust accordingly.

Possibilities of Extending the Curriculum to Include Behaviors Other Than Academic Progress

As mentioned earlier, it is easy to sell people on the idea that being smarter is good without even mentioning possible side effects which could occur in the personality-emotions-motivation domain. It is not easy to convince people that personality-emotions-motivation attributes can and should be trained. In fact, the present writer must admit some concern would occur on his part if the school system proposed that his child would experience a curriculum designed to teach "positive emotions" and would request careful explicit statements about what that meant and what procedures would be used before letting the school proceed. On the other hand, the increased violence in the

schools, the failure of young people to reject drugs, and the threat of an increasing population requiring people to successfully live close together leads the writer to believe that we may need to investigate the possibility of including the training of positive personality-emotions-motivation behaviors in the curriculum.

Some people might well suggest that this has always been done in the early years. For instance, the kindergarten has been partially justified on the basis of teaching social competence. Unfortunately, however, there has been relatively little effort to be more explicit about how this training is done, or how it is evaluated, (i.e., free play with other children is the training and everyone succeeds). What is being suggested here is that one can and should be more explicit about attempting to define what kinds of personality traits (e.g., sense of humor) are desirable and what kinds of experiences can enhance their probability of occurrence. This also includes defining negative traits (e.g., hostility or anxiety) and determining what kinds of experiences inhibit their appearance. It may also include teaching children when to exhibit appropriate motivations and/or emotions. One particular place where this kind of research would be appropriate would be in integrated situations where there is evidence that lower-class black children learn more when attending classrooms with middle-class white children (e.g., St. John, 1970). Yet there is also evidence that violence in the schools occurs in this situation (Syracuse University

Research Corporation, 1970). Again, information about interactions between student characteristics and teacher characteristics as well as the methods used is a long term goal.

Theoretical and Methodological Problems

If one accepts the premise that theory directs research, then one of the reasons that educational interveners have not been as specific as they might be about expected effects of intervention upon the personality-emotions-motivation domain has been the relative neglect these areas have experienced in current educational and developmental theories. None of the proponents of major intervention programs (Montessori, open classroom, contingency management, Bereiter-Engleman) spend much time discussing the implications of their programs for personality development. Books recommending sequential organization of material (e.g., Gagne, 1970) or utilization of behavior modification (e.g., Sulzer & Moyer, 1972) tend to ignore personality traits or individual differences of any kind, possibly because they are interested in general laws and assume that personality is just another label for learned behaviors outside of the academic domain. This latter position is taken by Skinner (e.g., Skinner, 1968) who does discuss how to teach behaviors which can be categorized and given personality type labels (e.g., creativity). This assumption that all personality traits are learned is controversial and still an empirical question. In addition, the current major developmental theories have been more concerned with domains of intelligence and perception (e.g., Piaget, Werner)

than personality. These theories would be rather pessimistic about the success of any educational intervention which included forced training and ignored a maturation-readiness position but as mentioned earlier are not specific about hypothesized negative consequences in the personality-emotions-motivation domain. These theories would also agree with educational programs which rely on the child for intrinsic motivation rather than using extrinsic reinforcers but again are not specific as to expected personality or motivation problems from those programs which use extrinsic reinforcers.

Psychoanalytic theory would probably provide some basis for theorizing on the negative effects of intervention projects (e.g., anxiety or aggression) but, in general, the theory emphasizes effects of the home and interaction with parents. In addition, psychoanalytic theory has been losing its influence (e.g., no chapter on Freudian theory is included in the latest edition of Carmichael's Manual of Child Psychology, 1970) which many claim is due to the failure of efforts to produce empirical support.

One of the most influencial theoretical frameworks in both developmental psychology and intervention research has been that of S-R learning. Of course this is not one theory but instead consists of a number of theories that have grown out of the associationist-empirical tradition. This tradition has probably served to retard theorizing in the personality-emotions-motivation domain because as mentioned earlier it looks on behaviors

categorized under such labels as also being learned and following the same rules of acquisition as other behaviors. This tradition has also been very influential in the area of research methodology. The disdain which these psychologists have for correlational data and loosely controlled research has led to extreme caution on the part of other American psychologists. On the whole, the present writer believes this has been an extremely good influence on psychology. However, it may have led to too much caution in the area of personality. In an excellent article, Carlson (1971) by summarizing research done in two major personality journals (Journal of Personality and Journal of Personality and Social Psychology) points out that not only has there been relatively little empirical work done in the area of personality development (only 2 studies were reported in these journals during 1968 which included preschool children) but also that 78 percent of the studies were experimental in nature and the same percentage included only a single session with the subject. Although the present author has already pointed out that the number of personality studies using young children has increased, there is still a strong tendency to stay with using a single experimental session.

Psychologists representing other theoretical frameworks which are relevant to the present discussion (e.g., self or need theorists) could probably make valuable contributions in constructing and evaluating intervention efforts. This is particularly true for the specification and study of treatment by subject

interactions. The self theorists who have developed many of psychology's more positive constructs such as self actualization might well spend more time considering appropriate antecedents to healthy personality development. Although the self concept is often discussed by interveners, self theorists in both theory construction and empirical work have tended to use older subjects.

In summary, there is a need for more theoretical conceptualization in the area of personality-emotions-motivation, particularly with regard to educational intervention effects. Theory is needed to guide the specification of important individual difference variables, the construction of evaluation instruments, the planning of programs, and the analysis of possible treatment effects.

An important problem which may result from a lack of theoretical conceptualization is the paucity of instruments for measuring personality traits in children. One reason that interventionists often give for not studying personality variables is that there are no instruments that may be used for that purpose. An important part of instrument validation directly involves the underlying theoretical framework employed by the test constructor.

Methodological problems are somewhat more clearcut. There is a definite need to collect larger samples of individual children's behavior. With the advent of wireless transmitters and video tape recorders the possibilities are almost limitless. There is the danger, however, of collecting vast amounts of data with no idea of how to analyze it. The problem then is to

determine what behaviors should be analyzed, again a reflection of theory. In addition, there is the problem of protecting the child's and parent's privacy. Although it is possible to remove names from test papers it is quite another thing to remove faces or voices from recording equipment.

When our primary interest is in capacity or amount of information, it is possible to acquire the data with test materials inside the classroom (even this involves several important assumptions such as the child is motivated). When the interest shifts to the personality-emotions-motivation domain it may be desirable that an effort be made to gather behavioral data outside of the classroom. When effective personality tests are developed, they need behavioral validation since the inference is generally made that behaviors "in the real world" can be predicted. This of course leads to all kinds of difficulties but is necessary for answering questions about the behavioral generality of hypothesized personality traits.

Not only do we need larger samples of behavior in many situations, we also need data gathered over longer periods of time. The longitudinal approach must be re-emphasized and the number of such studies increased. Clearly if we are concerned with relatively permanent changes in personality dispositions, observation over long periods of time is necessary. In her article, Carlson is able to answer typical objections to longitudinal studies and argue convincingly for an increase in their number.

The utilization of the experimental paradigm in personality research particularly where negative effects are suspected is another problem. While it is all right to identify personality correlates of educational intervention, it is another thing to manipulate the suspected antecedents so that more confident statements can be made about causation if these correlates are negative. In some cases it must simply be recognized that the correlational data is the best we are going to be able to collect. In these cases there are some analysis techniques (e.g., cross-lagged correlation) which can be used to improve our confidence about causitive inferences.

The acquisition of appropriate controls is another difficult problem. As mentioned earlier, it would be difficult to point to any intervention study in which both the subjects and teachers were randomly drawn from a larger sample of school populations to compose the experimental and control groups. After the control group is selected, it is difficult to make certain that the parents of these children do not seek treatment identical to that of the experimental children (this effect has been labeled "horizontal diffusion" by Klaus and Gray). There are several other problems which occur whenever extended intervention is employed, including different types of subject attrition, parental permission, and monitoring the treatment. In general, the more relevant information gathered the greater the confidence one can have in inferences made.

One of the greatest difficulties faced by experimenters who have gathered large amounts of data is how to proceed with the analysis. These difficulties are increased when multiple measures on the same individual are taken over time or when there are multiple independent and dependent variables. This problem has been alleviated to some extent by the increased popularity of multivariate analysis techniques. While the conceptualizations and computational formulas have been available since the thirties, it was not until advent of the computer that it became feasible for them to be used effectively. This may mean that some experimenters will need further training so that they can learn when and how to use these techniques. Several summaries of how these techniques can be used in developmental research have already appeared (e.g., McCall, 1970; Nesselroade, 1970) and excellent text books are available (e.g., Tatsuoka, 1971).

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II. Infancy and Early Childhood: Research Needs in the Study of Socioemotional Development

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Any discussion of an aspect of infants and young children's behavior is immediately confounded by the problem that aspects or domains of human behavior are interwoven. Any attempt to unravel them may produce invalid results. It is similar to trying to understand the auditory system by studying single cell behavior. We strongly believe that this reductionist approach may be a serious handicap to understanding behavior. It is important that we attempt to derive new models of behavior so that the reductionist argument does not prevail.

It is impossible to divorce the elements from one another. The taxonomies used by psychologists, while useful, may in fact hide the real relationship between these two aspects of human behavior. Perhaps this can be seen most clearly in the work of Charlesworth and his work on surprise (1969). In these studies Charlesworth, working with infants in the first year, was able to show that when one violated an expectation of the child, in this case a toy disappeared by way of a trap door from the child's view, the child's response was one of surprise and often laughter. In this example I think we can see most forcefully the inability to separate out a cognitive or an emotional component; that is, the emotional component of surprise and laughter was itself cognitive. Recently we have been

exploring the mother-infant interaction believing that this interaction would be related to the child's subsequent cognition development. In one of the studies that is presently going on we observe the infant and the mother in the home for long periods of time using a checklist procedure (see Lewis, 1971). We were interested in observing the matrix of behaviors made up of a set of infant and maternal behaviors and have been interested in the question of who does what to whom and when. In the past we have viewed the mother-infant interaction as creating a motivational set in the infant having to do with the infant's feeling of competence, and thought that this could be related to some intellectual measures of development, such as a sensory motor task, Bayley, or at later ages the Peabody language task. To our surprise we discovered that the mother-infant relationship does not only involve the building of motivational construct but involves cognition as well. This can best be exemplified by an example. We found that when the mother responds contingently and with short latencies to her infant's behavior -- a cry -- the infant builds an expectation that its behavior has consequence. Thus, the mother not only responds to her child building trust, attachment, etc., but she helps create expectancies in her infant, surely a cognitive activity.

In short while we refer to socioemotional or personality, emotion, motivation (PEM) we must not forget that cognition (and cognitive processes) must always be included.

Measures of Socioemotional Development

In order to observe what has historically been considered socioemotional development, it is necessary from the outset to state that socioemotional development, like cognition, is one of those terms which serves a useful purpose in conveying a general domain but which needs to be specified. In fact, there would probably not be total agreement as to what aspects of an infant's behavior would be socioemotional as opposed to some other aspect (the preceding discussion tried to make this point by discussing the difficulty of talking about socioemotional independent of cognition). In any event, we shall specify several dimensions of behavior which we feel are relevant to socioemotional development and then explore some of the problem areas. Moreover one might wish to include such variables as cooperation, sharing, help-giving to others, both peers and adults. This is certainly not inclusive of all possible categories of socioemotional development.

In the discussion to follow we shall first deal with these selective types discussing some general research findings and problems.

In order to clarify the research done in these areas to date it is necessary to present each of these categories of socioemotional development separately. However, I think it important to keep in mind at this point that little or no information is available on the relationship of these measures to one another. Thus, it may well be that these are not independent

kinds of behavioral attributes, but rather constitute some kind of clustering. This is no small matter and clearly bears on the whole conceptual basic of this domain. This problem must be dealt with but it awaits measures and procedures for generating data on specific topics before we will be capable of finding the interrelationships among topics.

Curiosity

Curiosity has been studied under the broad heading of curiosity, exploratory behavior, and play. The work of C. Hutt (1970), Berlyne (1960) and Lewis (1969) are just a few examples of the research done with infants. It is one of the more cognitively related socioemotional variables that has been studied either by the observation of attention distribution in terms of exploring novel situations such as some of Berlyne's research, or play behavior in Goldberg & Lewis (1969) and Hutt's (1969) work or in attention distribution as in Lewis' (1969) work. The literature on infant attention is quite vast and rather difficult to summarize and if we were to include it in our discussion of curiosity, we would have to state that there are a considerable number of studies (see Kessen, Haith, Salapatek, 1970). Usually not talked of in terms of curiosity are the Object Permanence Scales developed by Hunt & Uzgiris and Corman & Escalona. These scales, developed out of the Piagetian notion of sensory motor intelligence, offer the child a series of tasks in which the young infant must find objects which are hidden from the child's view. While the child's performance on these tasks has normally

been considered to be cognitive in sensory motor intelligence, one might argue that the child's search behavior for the missing object involves curiosity motivation (here again we see the interaction and conflict between cognitive and emotional variables).

While curiosity has been extensively studied, it has usually been investigated from the stimulus point of view, that is, what are the stimulus dimensions which elicit curiosity? Most of the research effort has been committed to stimulus dimensions such as size, contour, volume, number intensity, etc. The results, while certainly dependent upon the particular experimental situation, seem to indicate that even from birth the infant is an information seeker. However, this does not directly bear on the issue of curiosity since the effects described may be a consequence of the "power" of the stimulus to attract the organism rather than the organism's interest. Thus in the study of this topic it would appear essential that we investigate organism stimulus dimensions. Novelty and familiarity are two such dimensions in that they must be defined by the interaction and past experiences of the infant and its world. Relatively little work using this approach has been undertaken although most recently this has received increasing interest.

Individual differences in curiosity are almost totally unexplored (see Lewis, 1971) and research on this problem is in much need. One must not lose sight of an earlier problem when studying individual difference in curiosity namely the intertwining of the various dimensions under study. For example there is

every reason to expect curiosity and competence to be deeply related. Infants and young children who feel incompetence are more than likely to be less curious since curiosity would appear to be under the service of understanding and assimilating/accomodating to their worlds. This is an active process requiring feelings of pay off if successful, feelings incompetent children may not have.

Surprise

There has been relatively little in the infancy literature dealing with the variable of surprise except for several studies by Charlesworth (see survey in Charlesworth, 1969) and a study by Lewis and Goldberg (1969). In the Charlesworth studies an object previously presented to the child disappeared in an unexplained fashion and surprise was noted on the part of the child. In the Lewis and Goldberg study surprise was observed in an attention distribution study where the same repeated event after six trials was followed by a new event on the seventh trial and observer recording showed a significant increase in surprise responses. In both experimental procedures surprise seemed to be associated with subsequent positive affect, although Lewis and Goldberg suggested that surprise could be followed by negative affect as well. In a recent study of Kagan and his collaborators (Littenberg, Tulkin, & Kagan, 1971) a mother left the room and entered a closet instead of leaving by the door. Observation of the observers showed an increase in upset as a function of the mother leaving by the unfamiliar door. Although not

reported by the authors, it would be interesting if there had been an increase in surprise. Except for these few studies there is relatively little work on the variable of surprise and one must again be cautious in considering surprise as solely socio-emotional rather than a combination of some socioemotional and cognitive. Surprise, like curiosity, is, of course, an example of the interaction of these two domains.

The measurement of this variable is rather amorphous and somewhat subjective; however it touches upon what may be an extremely important initial characteristic of most affective experiences. If as some theorists believe (Schachter, 1965) affective experience is the combination of arousal plus the prevailing environmental mood, then surprise may be the measure of that basic arousal system. Individual differences in surprise also bear on individual differences in temperament or regulating mechanisms in that some infants appear to startle more readily than others. Moreover once startled there are individual differences in ability to dampen the experience.

Fear

In the last 3-4 years some research has been undertaken to investigate infants' fear responses, most notably the work of Ricciuti and his associates -- Ricciuti and Poresky (1972), Morgan and Ricciuti (1969), and Ricciuti (1968), Scarr and Salapatek (1970) and a recent study by Lewis and Brooks (1972) as well as Rheingold and Eckerman (1971). Using a variety of techniques, these studies have been interested in investigating, both from a

developmental point of view as well as an individual difference point of view, children's response to a variety of stimuli designed to be fearful. In the Ricciuti and Poresky and Scarr and Salapatek studies a wide variety of stimuli, most of them nonsocial, were administered to infants and their responses observed. The nature of those responses is quite important and Ricciuti and Poresky have elucidated on some of the problems in the measurement of these responses. In the Morgan and Ricciuti and in the Lewis and Brooks studies, as well as the Rheingold and Eckerman, human subjects were used as the stimuli. It is interesting to note that one of the consistent findings, at least in the Morgan and Ricciuti, and Lewis and Brooks studies was that the male stranger in both cases elicited more fearful response than the female stranger. Although Rheingold and Eckerman raise an interesting issue over whether or not the use of the term stranger anxiety is appropriate, that is, they report few children who really, in their terms, show anxiety, but rather seem to be coy or shy instead, this investigator, as well as many others, has seen extreme stranger anxiety and fear in children and does feel that young infants and children do respond fearfully to strange human beings, strange being defined as not familiar. In a recent study Lewis and Brooks presented to the child not only a male and female adult, but a strange female child in order to see whether or not the infants were equally frightened. The results indicated that, although the child showed clear negative responses (only two or three of the children showed responses

that one could really call fear) to the adult male and female strangers, most children showed positive response to the exposure to the female child. This result seems to call for a reinterpretation of the notion of strangeness as being a determiner of infant fear or concern in that the four-year-old strange female child was as unfamiliar as the adult female. Fear is a central human affect; however, there is relatively little experimentation in this area. What needs to be done is more observational experimentation using a wider range of social and nonsocial studies.

Competence

In the last three or four years there has been a growing concern over the issue of competency. Although there is a historical background for this interest which can be found in the psychoanalytic, for example, under Adler (Ansbacher & Ansbacher, 1956), or in terms of R. W. White's thesis of competence (1959), in the psychological literature it has most extensively been dealt with by Rotter (1954) in terms of his notion of internality and externality and, in the animal literature, Maier, Sigelman and Soloman (in press) seem to reflect some of this concern with the issue of competence. More recently Lewis in a series of papers (Lewis & Goldberg, 1969; Lewis, 1967; Lewis, 1971), as well as Zigler (for example, Schultz & Zigler, 1970) have concerned themselves with competence or mastery in young infants. Lewis has argued that competence motivation is acquired extremely early and suggests that it is visible within the first three months of life (see Lewis & Goldberg, 1969). We have further argued that

the feeling of competence or mastery is derived essentially from a responsive environment, usually in terms of the mother being responsive to the infant's behavior. We have worked out a model which states that the more responsive the environment and the shorter the latency between an infant's action and a consequence in the environment, the more likely the infant will be to build feelings of competence. Perhaps an example of the kind of variable we think competence to be is in order. Construct a situation in which a 13 or 14-month-old infant is given a verbal command to carry out and which requires that he leave the room that he is in, enter another room and do something. What one often experiences when one sets up such a situation is the sounds of pleasure which emerge from the child as he successfully is able to carry out this activity, whereas the silent and despondent results when he is unable to carry out what he was asked to. These same kinds of phenomena in terms of other behaviors, such as smiling and vocalizing, have been talked about by others (for example, Schultz & Zigler). If one is to place some emphasis on the Coleman report, one would be moved to consider this one of the more important socioemotional variables in that it seems to have high prediction for subsequent achievement. Thus, an infant who feels that he can master his environment is more likely to be the infant who will subsequently be successful, both intellectually as well as in other means. Interestingly, a recent paper by Birns & Golden (1972) indicates that

one of the best predictors of infant intelligence at year 3 is the pleasure the infant derives in doing an intelligence task at 18 months. What is particularly interesting about the study is that the intelligence score at 18 months did not correlate with the intelligence score at 3, rather it was the pleasure in performing the task at 18 months which was correlated with the intelligence at 3. Although the report does not make clear, it is suggested that the pleasure in the task might be related to this variable of competence or at least the child's feelings of competence.

The research literature on competence motivation is rather broad if one includes research with adults. Under the aegis of Rotter's social learning theory much work has been done on internality and externality; however, almost no work has been done on competence or mastery motivation in infancy and this is particularly crucial. Nor, might I add, has much work been done relating competence or mastery behavior in parents with mastery or competence behavior in infants. Neither have been explored satisfactorily. Because this may be a "master" variable in the sense that it may control the expression of many other socio-emotional variables, it is essential that it be studied in detail, its ethology, developmental course and consequence.

Humor, laughter and smiling

The smiling response literature has received considerable attention in recent years with Ambrose (1961), Kagan (1967), Lewis (1969) and Gewirtz (1965) observing children's smiling responses

to facelike stimuli. Gewirtz's work is particularly interesting in that the smiling response was found to vary as a function of institutional care. Humor and laughter, as socioemotional variables, have also recently received some consideration. McGhee (1971), in a review of the literature on the development of humor, has talked about the variety of theories: the psychoanalytic, Gestalt, arousal and cognitive theories. Certainly a major amount of work has been done on the cognitive aspect of humor (see, for example, Schultz & Zigler); however, there is relatively little work with infants. There is very little research on laughter, what causes infants to laugh and how this changes over age. An exception is a recent study by Sroufe & Wunsch (1971) who showed rather interesting developmental sequences in the kinds of stimuli and situations which cause infants to laugh. We have tended, somewhat glibly, to connect humor, smiling and laughter into some single category. Whether or not this is true remains in the theoretical realm; however, it is important to consider that at least in infancy these may not be necessarily elicited by the same stimuli or, in fact, under the service of the same socioemotional systems.

Even less theoretical are ways of measuring these variables and individual differences in them. Almost buried in the theoretical considerations is the interesting question of individual differences in happiness. While we are willing to consider the affects of humor, laughter, and smiling we rarely contemplate exactly to what internal state these behaviors refer. Happiness

probably comes closest to this definition. Could we use a happiness scale?

Attachment-dependency

The area of socioemotional development that is most investigated is attachment-dependency behavior. Historically, the attachment literature grew out of the psychoanalytic theory with Bowlby (1969) and Ainsworth's (1963) theoretical position being most widely accepted. More recently the work of Rheingold (1963) and Rheingold and Eckerman (1970), Hinde (1966), Lewis (Lewis & Wilson, 1971; Lewis & Ban, 1971; Ban & Lewis, 1971), and Maccoby (1971) have all contributed to the attachment literature. Attachment has been defined as a "category of behavior through which a discriminatory, differential, affectional relationship is established with a person or object" (Ainsworth, 1964), whereas Schaffer and Emerson (1964) state in a paper that "attachment is a tendency of the young to seek the proximity of certain other members of the species." These definitions have much in common and suggest several issues. First, behaviors which lead to this attachment effect have not been thoroughly described although it is suggested that those parent and infant behaviors act on one another, that is, both infant and parent become attached; and second which specific behaviors are characteristic of attachment have not theoretically been made clear. The whole study of attachment is in a rapid state of transition since under the thrust of a series of papers by Lewis (Lewis & Ban, 1971, Ban & Lewis, 1971, Lewis, 1972) it has become increasingly clear that

attachment as a unitary concept needs to be considered and that the behaviors associated with attachment are open to question. Recently (1972) we have proposed that attachment behavior in the opening years of life remains relatively stable between parent and infant; however, the behavior in the service of the attachment undergo developmental transformation. Thus, it has been our argument that the form of attachment goes from proximal body contact to distal, mostly visual regard and vocalization. Moreover, we have found important sex and social class differences in this attachment relationship.

Attachment and dependency is a particularly crucial domain in the socioemotional development of the child when we consider that one of the major functions of intervention is, in some sense, to separate or disrupt the normal mother-infant daily relationship. Disrupt here does not necessarily refer to a negative quality for, in fact, we may discover that certain kinds of intervention procedures in infancy facilitate subsequent socioemotional (attachment) development. What we do mean to stress is that to some degree the attachment behavior to the mother and to multiple caretakers has been explored but its subsequent effects are only vaguely understood. In several recent papers we have attempted to observe in great detail the mother-infant interaction in the opening months of life (Lewis, 1971) and tried to relate this to subsequent attachment behavior on the part of the mother and child, as well as to relate it to the child's nursery school performance. Attachment behaviors of infants in the first and

second year of life were obtained, as well as their nursery school behavior when these children were nearly four years of age. Over 60 children were seen in this experiment, and although the data are not fully analyzed, there do seem to be some rather interesting differences emerging which relate early attachment behavior to later nursery school behavior. For example, we have observed that infants who were moved quicker from a proximal to a distal form of contact with their mothers by one year of age showed, at four years of age, more physical aggression toward their peers and toward adults. Moreover, we find a fairly complex relationship between early attachment behavior and subsequent dependency behavior on the part of the children in the nursery school. In general, what appears to be emerging is those infants who are moved quickly from all forms of attachment, mostly, however, proximal forms, show more dependency toward adults at later ages. Thus, unlike what the reinforcement learning theorists might tell us, children who are allowed more physical contact with their parents do not end up wanting more physical contact with other adults. This finding is in the line with Bell and Ainsworth (1970) and their recent findings on responsiveness of infants' crying. They found, again unlike what learning theory would predict, parents who were more responsive to their infants' crying did not end up with infants who cried more; rather, they cried less. This suggests that there may be some basic need levels which need to be satisfied, and if satisfied do not result in greater habit strength but rather result in

the dissipation of the particular kind of need. This kind of theorizing, of course, is much more in line with psychoanalytic notions of basic need systems. It might also be pointed out that there is almost no work on the attachment relationship of the infant to its father. Except for two studies one by Rebelsky & Hanks (1971) and one by Ban & Lewis (1971), there is hardly any work in the literature of the father's relationship to the child in the opening years of life. In our study of children at about a year, we found decided differences in the infants' behavior toward their parents as a function of the sex of their parents, with both girl and boy infants showing much more physical contact toward their mothers than toward their fathers.

Thus, while the attachment problem has gained considerable attention recently, the problems associated with its study are quite complex. First, there is the general issue of how to measure attachment. Does one measure it by the child's response when the parent leaves? Is it measured by the response to the parent when the child is in its presence? It is apparent that both are valid measures and, in fact, Maccoby (1971) reports some consistency across situations. Second, what kinds of measures should we include under the domain of studying attachment behavior? We have suggested that one must consider a wide variety of responses and have suggested that a dimension that needs to be explored is the proximal and distal forms of attachment. Another problem that needs consideration is the relationship of attachment and subsequent peer relations. Still another important

issue, both theoretical and substantive in nature, is the difference between dependency and attachment. The attachment literature, of course, grows out of the ethnological school exemplified by such people as Hinde and Bowlby, who, for example, have argued that the basis of attachment is, in some sense, an imprinting procedure. Learning theory, on the other hand (see Cairns, 1966, for example), has argued for the dependency-learned drive accounting for the infant-parent relationship.

As yet unanswered is a series of questions dealing with the relationship between attachment-dependency in the early years of life and subsequent socioemotional behavior, both toward peers and adults. Longitudinal investigations are most called for since the length of time of study, at least for some questions about early effects, in nursery school, etc., can be quickly obtained.

Finally we must consider both from anthropological and sociological as well as psychological points of view the effects of different types of socialization systems. To claim that there is only one system is first to deny that there exist across cultures and time varied systems and to deny that there may be multiple paths to similar goals. It will become increasingly more important as we approach these issues to rid ourselves of the simple notion that there are either good or bad socialization processes. Rather we must define a set of goals and a set of processes and determine the relationships between these goals and the methods for obtaining them. Thus for example multiple

mothering may facilitate peer and retard adult attachments while single mothering may cause the reverse. Each of these goals are different goals for some and each require different types of socialization.

Anger and frustration

The research literature on response to frustration and anger in infancy is almost nonexistent. Lewis (1967) reports some data of frustrating infants by removing the bottle from their mouths during a feeding and relates that this behavior was subsequently related to the vigor of their attempt to knock down a barrier at a year of age. However, a recent study by Bell, Weller & Waldrop (1971) tended not to find the same effects as reported by Lewis. Other than this there are relatively few studies on infant response to frustration or infant anger. This is somewhat surprising in that frustration and anger are variables that have been widely studied in the animal and adult human literature. Its absence in the study of infants probably reflects the reluctance on the part of experimenters to initiate unpleasant or negative experiences to the young infant. While we would tend to agree with this position, it seems quite feasible to observe the infant's response to naturally occurring frustrations in its environment and the children's attempt to overcome these experiences. It would appear that the infant's response to frustration, its anger and its attempt to overcome this frustration might be related to some of the other variables we have discussed, for example, competence and mastery. Anger, although not a

positive dimension, should be studied for many reasons, one, of course, that it is a dominantly occurring emotional experience in human lives, and the intervention experiences which we conceive might affect the development of anger and frustration. Moreover, one might hypothesize that in order for certain positive aspects of coping with the environment to occur it is necessary for the infant to experience and learn to cope with anger and frustration. Thus, although there is a little literature on this subject, this dimension of socioemotional domain is worthy of consideration.

The concept of self

Much of what we have been referring to can be subsumed under the concept of self. Can one talk about the concept of the self at such early ages? Consider two aspects of the self: the first and most common is the categorical self (I am female, or I am intelligent, or I am big or small, or I am capable); the second, and by far the more primitive, is the existential statement "I am." The basic notion of self -- probably as differentiated from other (either as object or person, the mother being the most likely other person) -- must develop first. There is no reason not to assume that it develops from birth and that even in the early months some notion of self exists. We would argue that this nonevaluative, existential self is developed from the consistency, regularity, and contingency of the infant's action and outcome in the world. Self is differentiated by reafferent (or information) feedback; for example, each time a certain set of

muscles operate (eyes close), it becomes black (cannot see). That is, the immediacy, simultaneity, and regularity of action and outcome produces differentiation and self. The action of touching the hot stove and the immediacy of the pain tells me it's my hand that is on the stove. This self is further reinforced if, when I remove my hand, the pain ceases. The infant's world is full of such relationships and they vary from its own action on objects to its relationship with a caregiver. In these social interactions, the highly directed energy of the caregiver (touch, smile, look, etc.) is contingent and specific to infant action (smile, coo, etc.).

The relationship of self to the responses to a mirror is a clue. Data from a variety of sources indicate that looking in the mirror is pleasurable. This is because of the consistency, regularity, and contingency of the viewer's action and the viewed outcome. In no other situation is there such consistent action-outcome pairing. In other words, the mirror experience contains those elements that generally make up the fabric of the infant's growing concept of self. It is not possible for us to know if the infant is aware that the image is himself. Awareness is a difficult concept to study in nonverbal organisms, but it is clear that by the time one-word utterances emerge, such as "self" or "mine," the year-old infant has the concept of self. It is reasonable to assume that the concept existed prior to the utterance. In fact, if we consider the research on the development of object permanence (for example, Charlesworth, 1968), we find that, for the most part, object permanence has been

established by 8 months of life, in many cases even earlier. If the infant has the cognition available to preserve memory of object no longer present, how can we deny them the ability to have self-permanence capacity? Given that this first self-other distinction is made very early, the various categorical dimensions of self may also proceed to unfold. The unfolding of the categories, whether sequential, hierarchical, etc., and the dimensions of the various categories are uncertain.

For example is our understanding of the phenomenon of fear helped by evoking the concept of self? We would argue, yes. Hebb's (1949, p. 243) study on the fear of monkeys could be explained by this concept. Consider the monkeys were fearful because they saw a monkey without a body and they were aware that they, too, were monkeys. Maybe they too could lose their heads to a mad professor. Would not humans placed in a similar situation show fear for <u>their</u> lives or safety? The Gardners report that in their study of sign language in the chimpanzee, the animal exhibits the concept of self. When shown a mirror Washoe responded with the signs "me Washoe." Thus, it is not unreasonable to attribute the concept of self to other primates.

Sex differences in fear as reported by Morgan and Ricciuti might likewise be explained. The specific category of self in this case may be gender. The Money, Hampson and Hampson (1957) data on sexual identity suggest that a year-old infant may already possess this category. That in our study infants were not frightened of strange children suggests that another categorical

dimension of self may have to do with size. The concept of self and its development must be further studied for it is integrally tied to the attribute we described. In fact, there are attributes of self.

Trust

The concept of trust as introduced by Erikson (1950) would appear on the surface to be related to or reflected in the attachment behavior of the infant to its parents. However, trust in some broader sense seems to be an important socioemotional variable. However, like anger and frustration there is almost no work in the infancy literature to reflect this variable. The infant's capability in terms of trust may be extremely important in terms of its learning to delay gratification or in terms of its learning to inhibit impulses. It seems reasonable to hypothesize that the infant who is capable of experiencing trust can watch someone showing him how to do something with a toy without having to reach out and grab the toy in fear that he will never get the object. This inhibition of reaching and the capability of listening while in the presence of an attractive toy certainly should facilitate educational experience and may be a parameter of the dimension that we call trust.

From such a brief discussion it becomes clear that we have just begun to understand the infant and young child's socioemotional development. A few studies here and there do more to tease us than to light the path. While some gains have been made in the understanding of early cognitive development, the socioemotional realm has remained almost unknown.

There is good reason for this, for it mirrors the problem in all of psychology not those to do just with early development. In cognition we have some good idea what constitutes the end point of development and we know what we wish in terms of our children's growth. Thus, if you ask a parent what cognitive skills they wish for their child they will report, "writing, reading, reasoning, problem solving and perhaps even creativity." The same question about socioemotional development evokes a considerably more varied and confused set of answers.

This confusion pervades all of American psychology because the path we have chosen, at least until recently, has been behavioristic; a path which allows little for the feeling states. If we wish to enforce this domain of human experience it will become necessary to reject in part both the behaviorism and the reductionalism which prevents us from exploring these problems. For example, what is the relationship, if any, between a set of lip muscles, smiling and happiness. That is the question. We must turn our attention to the underlying states and not the observed behaviors per se. Smiling is a good example. We smile when we are embarrassed, happy, angry and frightened. How can we use the behavior to infer state?

This problem can be explored if we are willing to reintroduce a live subject into our experimentation; phenomenology -- the subjects experiences are worthy and necessary to utilize in solving this problem. Unfortunately this technique is not available to those studying infants for infants are not capable of

verbally expressing these feelings. By working with older children we can approach this problem using the results as inference to the behavior of the very young.

There are several other problems which need to be mentioned in this type of discussion. While implicit in our discussion of some of the socioemotional variables, the dimensions of intensity and frequency need to be made explicit. It would seem unlikely that infants and young children would fail to show an attribute; thus no infant would never smile. Moreover, two infants might smile the same amount of time but one was clearly more intense. It would seem likely that instead of turning our attention to presence and absence of an attribute it would be more profitable to scale for frequency by situation and intensity.

Still another implicit problem has to do with what we shall call short or long term attribute. It may be possible to demonstrate that given a set of conditions both children will laugh; are we willing to say therefore that both children are happy? In most all of the research we have concerned ourselves with the demonstration that a child has the attribute, clearly a trivial matter. What really needs to be studied is individual differences in the long term feeling states underlying the attributes themselves which are not situation specific; i.e., the personality characteristics. Thus, it is not what makes a child full of fear but whether the child is fearful; not what makes the child smile but whether the child is happy!

This problem domain (PEM) is difficult to explore in adults when all the available technological, experimentational and verbal facilities are available. How much harder will it be when we have none of these and when we must reject much of what we have thought about these problems in the past. Observation and more naturalistic observation is the answer. It seems clear that we must begin by putting away old conceptions and by getting down on our hands and knees to look again!

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III. The Status of Research of Social-Emotional Development

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To say that the field of psychology in general and developmental psychology in particular, is beset with style and fads of research interest would not be very novel and something that each of us has taken for granted. For any of us who have received our doctoral degrees in 1952 or 1953 and earlier, know that within the last twenty years the range of interests and emphases for research in the field of developmental psychology has ranged wide and far. What is taken for granted today as to points of interest were not even mentioned in 1951 or 1952. The tragedy of this type of fadism is that our science does not progress in an orderly fashion where knowledge is built on previous knowledge, so as to result in the year 1972, in an array of organized systematic knowledge. The dynamics of this seeming disarray are something worth studying in their own right. The tragedy is that in this disarray delays in development of the science occur.

I believe, however, that the time is right for innovations to occur because as social-emotional research reached its peak in the late forties or early fifties, so too, I believe that cognitive development as such, is also peaking and we are in the process of a new synthesis. We have rediscovered the fact that in spite of man's reasoning capability, these processes are

imbedded in an affective context so that emotion and reason are intimately intertwined and both provide the dynamics for subsequent behaviors. Further, we are beginning to pay more than lip service to the ecological determinants of behavior. Not only are we interested in the ecological environment, in terms of pollution, but we also become interested in describing, in ecological terms the settings in which particular behaviors occur. Thus, we are becoming increasingly aware of the interaction between the domains of cognition, emotion and situational context. The reasons for these would require much more time and thought than at the moment, may be useful. But, it would certainly be of inestimable value if one could or would devote some time to an in depth analysis of the shifting foci of interest that are expressed periodically in our field.

In the next decade it seems to me that we are faced with three major problems which center on (a) the conceptualization of development; (b) the instrumentation and operational definitions for study; and (c) the methods employed for data analysis.

In our orientation to science and our orientation to the specific subject matter of developmental psychology, we have developed a style of investigation which includes the development of theoretical statements, the definition of constructs, the derivation of hypotheses, and testing of the latter by various empirical methods. This sequence of events, and the investigative model, drawn largely from the natural sciences, must not be overlooked. Indeed, a note of caution should perhaps be offered

lest we let the model of science that we have heretofore developed become a stranglehold that prevents our thinking of new methods or ways for defining and establishing the types of data that are of interest to us.

In our conceptualization of the developmental process and its complexities, we are similar to the individual thrown in a jungle with an array of exciting options, beautiful plants, dangerous animals, wonderful trees and varying degrees of sunlight and shade. How one works in this maze depends on how he elects to organize the array of stimuli before him. In my analogy, the use of such adjectives as beautiful and dangerous and interesting already suggests a categorization of the arrays of stimuli. So too, when we look at the developing child. There is no question that the child lives in the home environment and then moves out into a variety of other environments, each of which impinges on his development and has some impact on the nature and directionality of his growth. All of us as human beings know that we think, feel, and introspect, that we have attitudes that we express and behaviors that we engage in that are contrary to our feelings. Therefore, we have regrets and thus distresses and tensions. But, we also know that when we alter the directions of some of our activities, we can cut short the insights as to what we should or shouldn't do. In other words, we ourselves become or can become an important source of information as to what the nature of humanness is. I firmly believe that this source of information is of an estimable value in attempting to order the array of situations in front of us.

On the other hand, it is also very terribly dangerous because each of us has his unique way of organizing himself, in spite of the commonality we have with others. In fact, it is this commonality that enables us to justify research using descriptive statistics, because in essence, such research implies that characteristics existing among humans are generalizable and that in spite of each uniquenesses, we do share a tremendous amount of commonality.

With this framework, we then come to the question of the need to conceptualize the organism in an organismic way, but whether one wishes to speak in terms of behaviors or mentalistic terms is not at the moment the issue. The problem is that the organism is a complex unit of integrated parts which do not function in isolation from each other. The response to an operant procedure has an affective tone. We do not function without feelings, we do not function without energy, we do not function without various states of tension. Thus, in our conceptualization of behavior, we must somehow come to grips with the reality that man's behavior is response to a number of concomitant states, each of which is capable of mediating the consequences of the other. Thus, for example, one does not fall in love with certain people because of taboos. This is a cognitive impact on the control of the affective system. One wants to do something, but realizes the consequence and doesn't engage. This is the influence of cognition on affect. One feels attracted to somebody and tries to figure out a way to approach him. This

is where cognition operates in the service of affect. Consequently, the conceptualization for me that is very central in the next ten years is to develop a conceptual system which allows for the integration of the affective state with the cognitive aspect.

This, then, is going to require constructs that may not be the ones that we have currently used. In the present state of research on social-emotional development, we tend to focus on such motivational dimensions as aggression, achievement, dependency, or similar need-drive systems that have been the target of research investigators for many years. Valuable as these are in defining conventional behavioral domains, we also have become increasingly aware of the looseness of these definitions as well as the intertwining of them. For example, one can speak of anxiety which arouses one to be aggressive. One can speak of aggression which is expressed by virtue of dependency. One can speak of dependency which is expressed by aggression. The various levels of affect and their intertwinings require, in my estimation, very careful systematic thought. It is not the designation of the variable or the dimension that is at issue, but rather it is the level of relationship and the model of expression with which we work. Thus, for example, the concept of one affective domain functioning in the service of another is a case in point. Aggression related to anxiety affects its expression which may lead to inhibition of the expression and, therefore, to depression or tension. On the other hand, one can

turn all these things around and talk about a vague sense of anxiety which the individual wishes to release by destroying what he thinks is the object of this anxiety and now, we find that the individual is using aggression in order to allay anxiety where heretofore, we have seen the anxiety as a function of the individual's aggression.

A conceptualization, then, of the levels of interaction becomes very important, and should lead I think, to conceptualizations that move away from the univariate approach and speak to the interrelationships among events. I believe that one of the most inhibiting factors in the development of our understanding of the role of social-emotional factors has been our tendency to view these variables in a non-interactive-linear univariate approach. I feel this accounts for so little of the variance, especially, if we move outside of the laboratory, that our knowledge becomes much too restrictive. Thus, I am proposing a reassessment of constructs and the development of new constructs that move us toward actual interactive statements which speak to the complexity of the variables in question.

In this section, I would like to provide some specific examples of the issues discussed above.

Problems dealing with the interactive effects of various personality characteristics:

We have found in our nursery school study that socially outgoing children who attended to tasks and who related to examiners performed better on cognitive tasks than children who

were quiet, cautious and withdrawn from the examiner. These personal-social variables form a cluster of characteristics that related to test behaviors. Further work of this type is in order, creating other clusters and performing multiple regression analyses not only to predict a single dependent variable, but a cluster of variables, e.g., perceptual-motor skills.

Secondly, these predictions are in a contemporaneous time point. I would suggest that clusters be created for long-term prediction to determine the consistency of such behaviors over time, i.e., given a cluster (e.g., outgoingness, attention, relating well to examiner), what variables (singly or collectively) will it predict a year later, or two years later, etc.?

This leads to the issue of stability of personal-social traits. The stability of traits or characteristics is a crucial theoretical and empirical problem. Clarification of the construct of stability is also crucial. In other words, the criteria of stability must be explicated. For some characteristics the trait may be consistent, e.g., activity level, over time. For others, the characteristics may be transformed, e.g., aggression into anxiety, for example, while for others they may drop out to reappear at a later time, e.g., dependency. The tracing of personal-social characteristics over time creates some complex and challenging methodological and data analysis problems.

Factors which mitigate development of personal-social characteristics must be identified. These can range from physical factors, injury, and disease to environmental factors, e.g.,

family moving, loss of parent, change in family status, etc. Differential rates of growth further complicate the matter, since some characteristics may well be moderated by temperamental variables. There have been some studies here, (Emmerich, Stott, Kagan). These are all limited in scope and in terms of populations involved. Replications and extensions to other clusters of variables are in order. The assumption that findings on one population are generalizable as universal characteristics cannot be assumed. Parenthetically, definition of population characteristics becomes a critical factor in mitigating validity of generalizations. For example, variations in child rearing patterns may alter the interrelationships that are found. Further, particular social behavior, functional in one setting, may be dysfunctional in others. Thus, cautiousness or suspiciousness may vary as a function of social ability in different social strata.

The clusters have to be empirically defined and here we come to the definition of relevant variables. A crucial dimension, defined by "locus of control" variables, represents the degree of conscious awareness on the part of the individual that he can master and manipulate his environment. Assessment of children's behaviors and beliefs regarding this in the classroom is crucial.

A critical feature of the "locus of control" issue which goes beyond the definition of the dimension is its "reality." One can be in a better position to determine one's fate regarding

going to the movies or working than of controlling one's economic fate. The issue is one of generalization across contexts or to put it another way, of specifying conditions in which particular behaviors are predicted to occur.

From here we become interested in antecedants of the phenomena in question. In the case of locus of control, for example, one might ask what parental behaviors are related to the quality of the development for such controls. Locus of control is a complex concept admittedly in need of conceptual clarification. However, the general problem area seems to be of considerable import.

A word about research strategy. We are approaching a period when multivariate procedures are becoming more and more de rigeur. The need for such an approach is considerable, especially in the socio-emotional area. The complexity of the motivational system and the obvious interactive effects of social and emotional factors demand analysis more closely expressive of these interactions. Examination of configurations of characteristics is another way to seek a detailed understanding of relationships.

A second important aspect of research strategy concerns the interrelationships between affect and cognition. Cognition is not independent of affect, and affect does not operate without knowledge. As Piaget holds, these are both sides of the same coin. The relationship between cognition and affect in terms of their influence on directionality, quality, and concomitant behavioral development is extremely important. Thus,

relational constructs are necessary for us to move ahead and deal with significant issues in these conjoined domains.

One construct that expresses this conjoined quality is cognitive style. Irrespective of the conceptualization (at this point there are a number, Kagan, Gardner, Witkin, and Sigel) they all incorporate affective components by implication and explication. What is needed, in addition to more precise conceptualization, are refined methods which would allow for extensive developmental study.

For Piaget, interest and motivation are always involved. How, then, can one conceptualize cognitive tasks independently of motivational aspects. The individual's interest in and attitude toward objects, events, and problems, etc., should have some effect on performance. If this is so, categorization, for example, of noxious or attractive materials should have different effects. The definition of the "attitude" toward a set of stimuli then could be examined independently to determine the degree to which it influences performance; to put it another way, "Does the cognitive processes transcend the affective components and if so, under what conditions?"

If cognitive style moderates how one approaches particular objects and events, then this approach will influence the information that is obtained and how it is used. Attention to individual differences may well divulge the relationships we are discussing. Approach here can refer to the tempo involved, e.g., reflective, impulsive or to the aspects of the object attended to.

Finally, considerable work needs to be done to grapple with the problem of the cumulative affect of the cognitive-affective interaction. For example, suppose that a child learns about his efficacy in causing an object to break, and is punished for this act on a number of occasions. What he learns about the consequences of his actions become part and parcel of the individual's experiential base, presumably influencing subsequent behaviors. The cumulative impact may well influence each subsequent learning experience. In the course of this, however, memory comes into play. How these experiences are stored and retrieved may well explain how subsequent acts are influenced.

The model I am working from argues that experiences are integrated at varying levels of development. The quality of the content of this integration depends on the type of experience. All experience involves some affective discharge, and these "feelings" become attached to the learnings involved. Hence, retrieval of particular knowledges for use in subsequent settings is probably influenced by the atmosphere in which the knowledge was acquired.

A third area of study which is related to the intertwinings of cognition and affect is that of the situational context. We are all aware of the role of culture and the tremendous problems of assessing social-emotional development on a cross cultural basis. We have been concerned with this for many years and much work has been done on cross-cultural, personality development, and the like.

In the past, situations have been defined in rather gross terms, e.g., urban-rural. Within the past decade Barker's concern for investigation of the ecological factors influencing behavior has received increasing attention. The work of Sells, Willems, and Sarason provides examples of what needs to be done. We must do at least two things here: 1) work toward increased precision in identifying situational dimensions so that conceptualization of the environment can advance, and 2) begin to particularize situational variables in describing our research. The degree to which generalization can be made independently of ecological factors is a crucial issue. Laboratory research does not provide the answer to many questions. It merely identifies a problem and indicates results under a given set of circumstances.

The growing interest in this area speaks for itself. The rationale is clear. What does create a problem is the fact that until the dimensions are defined with some certainty, we have a long way to go in this field. However, the fact that generalizations about behavior and its relationship to the ecological factors are being considered in a broader context is a step forward.

The organization of ideas, such as have already been touched upon, must be put into a developmental context which goes beyond the changes which occur over time, and includes various kinds of developmental interrelationships. The concept of development has to be changed from the linear type of cumulative model, as described by people like Bijou, Baer, Spence, and Skinner, and must deal with a more complex view of development which includes

not only behavioral aspects, but also mental states and capabilities. I feel the behavioristic model of development is limited and naive and tends to overlook the complexity of structural change that occurs in the organism. But, unless we have some progress in our conception of development, we will always come up with compromised limited understandings of the process.

Another area of consideration, of course, is the need for instrumentation. Here I am preoccupied with the need for adequate measures, but am fully aware that measures, no matter how skillful we make them, may never tap just the behavior in which the investigator is interested. In developmental psychology we are forced into measures which are very behavioral with young children, such as observation schedules applied to different situations. But other observations in test situations in different contexts using clearly defined concepts become very central. We have an array of such measures, many of which are very adequate, but which we never bother to codify or to use other peoples' measures to see if they work. We almost always invent our own. Perhaps the title of this paper should have been "The Re-Invention of the Wheel Number 99," because we're always doing this and we're always saying we're doing it and we proceed also to continue doing it. This is a good case of where we're doing it, we're articulating what we're doing and what's wrong with it, but it doesn't alter what we're doing. The problem of instrumentation is complex , but it is something that I feel we must really attend to. There are hundreds of rating scales,

tests, observation schedules, and experimental situations for investigating such things as self-concept and achievement. All of these are available and many of them may be very useful. What may be needed is time, effort and funding for developing the kind of instruments which would merit the same use that the Stanford-Binet or the Peabody Test has achieved.

The characteristics of measurement instruments have been alluded previously, but herein arise a number of questions. First, the validity of assessment procedures should not rest solely on indirect criteria, such as would be the case if a new I.Q. test were considered to be valid simply because it correlates with the Binet. Much more thought needs to be given to the validating criteria.

Second, conditions of assessment must encompass all the attendant related behaviors. Some of our own research has demonstrated that social interactions do influence test performance. Rating scales to be used by examiners, for example, can be extremely valuable if they involve relevant items. This will necessitate more precise empirical research.

Research must not only be integrative and generalized, but also must have predictive validity for significant conditions in life. It seems to me that validation will be enhanced to the degree that we are able to deal with configurations and patterns of behaviors, rather than the single variable operating as though other conditions are controlled. This is a horrendous problem, but I think we are in a position now with computers and

good programmers to deal effectively with configurational analyses, and hence to examine configurations in their dynamic interrelationships. By configurational analyses, I mean development of profiles of attributes (e.g., cognitive, personal-social) which extend our knowledge of the person. Thus, assessment procedures have to be employed in batteries that provide an opportunity to develop an individual profile.

In the preceding discussion, I have said very little about specific studies, constructs and measures. Instead, I have asked for greater clarification and definition within particular problem areas that I have defined.

IV. Ecology and Development: Future Directions

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Despite the large number of reports purporting to evaluate educational programs designed to provide "deprived" children with those skills required for school success, it remains unclear whether these programs have been successful. A major part of the problem can be attributed to faulty design and data analyses but some aspects of the problem relate to poor conceptualization and lack of specific knowledge relevant to the culture of minority people, especially in terms of their behaviors towards their children. In this paper an effort will be made to delineate these problems, show how they confuse our thinking, and suggest fairly obvious research programs which will provide the knowledge base required for more informed policy decisions and programs.

One of the more important, or at least influential, hypotheses developed to "explain" performance differences between minority groups and the middle class has been labeled the "cultural difference hypothesis." This position is most frequently attributed to Labov (1970) who concluded, on the basis of sampling Black lower-class language, that their language is at least as structurally complex as that of the standard White middle-class only it is different. He concludes, appropriately, that any interpretation which suggests "deficit" is incorrect and fails to allow for obvious cultural variations in language forms. It should be noted that Labov is <u>not</u> responsible for pushing the

"cultural difference" hypothesis beyond language but rather one can find that case developed by Cole and Bruner (1971). Thus the position is taken that the poor performance on a variety of tests of lower-class children, Black and White, is a function of their different cultures and not a reflection of deficit. One does not need to accept Jensen's (1969) position to understand the circularity of this interpretation; note it is equally fallacious to argue that lower test score performance reflects either a cultural difference or an aptitude difference - the antecedents are simply unknown!

There is, of course, a more important error in the thinking of those who interpret all findings with respect to SES difference in terms of culture. Specifically, there is the implicit assumption that <u>all</u> members of a particular social class possess the <u>same</u> cultural features very much in the way that uniformed interpreters of test data conclude that all members of a race or class are <u>equally</u> deficient. In point of fact, the overlap in distributions of test scores between classes or races far exceeds the differences between the means of the groups. One can only assume that a similar situation prevails with respect to cultural values, and their attendant behaviors, especially those that are hypothesized to influence both social and academic performance in the schools.

This lengthy introduction to what seems to be an obviously important problem is prompted by the fact that its equally obvious solution is time consuming and expensive. What is required is

a program of ecological research along the lines long advocated by Barker and Wright (1955). Their use of observation procedures is widely known and the implications of their data for informing us of the enormous number and variety of encounters in a child's day have been of extreme importance to the study of child behavior. Despite the contribution of this work, it should be noted that their objective was to describe behavioral encounters rather than to employ ecological strategies in some form of hypothesis testing. A similar descriptive objective is currently being achieved by Schoggen (1972) both with respect to observations made of lower-class children at home and at school. Schoggen's data may provide a better picture than is now available of the home and school lives of these children but will probably not provide a satisfactory explanation of how these encounters influence development.

It would appear that the technology for conducting ecological research is available although there continues to be a problem of data reduction. But assuming that such problems can be resolved, there remains a much more complex set of conceptual problems. Perhaps the nature of the problem can be best stated in the form of a working hypothesis: There exists a set of identifiable variables in the domain of cultures which distinguish environments and which are related to those child characteristics influencing (related to) school performance, in particular, and adaptiveness to society, in general.

First, consider the initial component of the hypothesis: variables that distinguish between cultures. What is needed is

not a set of ad hoc conjectures based on casual observations but a comprehensive model. Such a model simply does not exist and existing data does not provide much in the way of clues as to how to construct a model. It should be made clear that what is needed here is not the identification of such macro-variables as mid-parent education or I.Q., job description, or neighborhood but the quality of interactions that these rubrics incorporate.

Any attempt at developing a conceptual model should include a developmental view both with respect to cognitive and social behavior. Thus in the study of any culture it is as important to know what a child experiences as well as when these experiences typically occur. It may be that certain experiences are, in actuality, crucial for survival and occur earlier whereas other, seemingly less important events, are less crucial and occur later. But, there is the possibility that overly delayed experiences can be detrimental for further development. Specifically, the developmental view demands an examination of both what occurs and when it occurs.

A basic premise of the required model must include the biological characteristics of the populations of children. The use of the term biological is meant to refer to those observable variables that are apparently related to developmental patterns and which have effects on school performance, both social and cognitive. Perhaps health is a better term than biological because some of the known variables include birth weight (for term), evidence of prenatal or perinatal anoxia, Apgar or Brazelton

ratings of neonatal vitality, and general nutritional status. These variables are known to have long term effects and to occur two to five times more often among the lower-class, especially Blacks. That these variables affect the nature of parent/child interactions was demonstrated dramatically in a study reported by Stechler (1965). In this study, using middle-class families, distinct differences were found in the behavioral and attitudinal patterns of parents where neonatal anoxia had occurred (approximately one-third of the sample of 27) and where it had not. The mothers of the anoxia babies were more negative towards their children, reported their children as having more eating and sleeping problems, and a higher frequency of tantrums. Test score data collected over the first three years of life revealed considerably greater between test variability than for the "normal" children. Essentially similar findings were reported by Birch and his colleagues (Thomas, Birch, Chess, Hertzig and Korn, 1963) and Bell (1972). Indeed, even such an obvious child characteristic as sex influences the quality of parent/child interactions, and not just in terms of sex-typing (Morse, 1967).

A moments thought about the potential interactive effects of biological variables with family and school variables should generate numerous possibilities for researchers and, it should be noted, these research questions contain important policy relevant issues. Consider, for example, the conclusion that anyone of the earlier specified variables accounts for a majority of the variance observed in school performance. This is to say, that

the biological antecedents, even when the interaction variance is removed, still stand out as the major predictors. In this unlikely event, the clear implication would support the placing of an even greater priority on medical research. Another and more likely possibility, is that the biological variables interact with certain types of parent attitudes and behaviors which, possibly, transcend socioeconomic status. Here one can speculate about dimensions of control (authoritarian - permissive), methods of coping with uncontrolled behavior (physical retribution - psychological retribution - no retribution) or the effects of the child's behavior on the marital partners. The character of these early interaction effects should change as their effects become manifest over time and it should be possible to describe later behavior outcomes more precisely. In terms of policy relevance, it should be possible to better prepare and help these parents and to provide day-care supervisors with similar information. Obviously, this outcome would still provide an empirical basis for retaining a high priority for medical research. There are certainly many other questions of this type which can be asked, depending on specific interests. These are complex questions and will require care in identifying covariates but the outcomes seem obviously worthwhile.

A second component of the model must incorporate some conceptual viewpoint of the nature of the developing child in order to identify what environmental variations warrant investigation. Here a choice is demanded between essentially structural

(organismic) and mechanistic models of man. The writer's bias is toward structural models first because they seem more consistent with his concern for biological antecedents and secondly because structural models appear more parsimonious for the given problem. The last reason perhaps requires comment. An organismic model, as I see it, views man as possessing innate structural properties that are invariant and which emerge over time given appropriate environmental stimulation. This view is essentially similar to Chomsky's (1967) with respect to language acquisition. It's advantage in terms of parsimony is that its focus is on how competencies are acquired which operate across broad spectrums of behavior classes. Thus, rather than having to account for an almost infinite number of behaviors, the structural view demands explanations with respect to a finite number of competencies (all of these competencies are not now known).

A concrete example of what the implications of this approach generates may be helpful. Without citing specific authors (there is no need to embarrass them) one can set down a number of specific stimuli thought to be crucial for normal development: crib-mounted mobiles, color-naming games, form-naming games, orderly dinner hours, noise level, space, books, etc. Of course these stimuli are useful (although there must exist an infinite variety of substitutes) but they rely on the child's attending (visually, tactually) to them. Lewis (see his paper in this report) has shown rather persuasively that lower-class children are less likely to attend to such stimuli; more accurately, the attend

later than their middle-class peers. Attending behaviors reflect basic processes that influence further learning (for new knowledge develops from already formed structures) but the issue here is to contrast how middle- and lower-class parents create situations that foster these competencies. Object-permanence is a competency that emerges during the infancy period which, I would think, could be stimulated in many ways. Perhaps a classic behavior is "peek-a-boo", or hiding a toy (as the baby observes) and asking him to find it. I doubt, but don't know, that these are universal behaviors. Are there substitutes and, if so, what form do they take? When are these behaviors typically used by parents and what are the reactions of the babies (do they visually attend, do they smile, etc.)? Assuming that there are content differences between classes, are there qualitative differences that are discernible that may account for variations in the age at which object-permanence is established?

Another important area of development is attachment for which at least two major theoretical approaches exist: ethological and social learning (Gewirtz, 1969; Bowlby, 1960; Ainsworth, 1970). It is not my purpose here to either review each position or to evaluate them. Suffice to say that both positions rely heavily on dimensions of the quality of mother-child interactions. The ecological position naturally emulates the organismic position taken in this paper and Ainsworth among others has made significant contributions to this area. However, Ainsworth's work (this involves babies between seven and 12 months) has

usually involved middle-class parents and, for the sake of experimental precision, laboratory contrived situations; a strategy not unlike that used by Hess and Shipman (1965). The problem here, discussed fully by Cole and Bruner (1971), is that the middle-class parent's reaction to the laboratory situation logically will be different from the lower-class parent's and thus influence the results. The parental correlates of attachment behavior have been reasonably well specified (at least sufficiently well to permit an adequate specification of observable parent/child interactions) so that systematic observational work could begin including SES related behaviors as major variables.

Another important source of process variables are available from Piaget's (1947) work with respect to the toddler age child.

Of particular relevance is the emphasis given to "play", particularly symbolic play. Although play continues to involve substantial components of motoric action, careful observation indicates that increasingly during the toddler period the features of the play include representations. Thus a pencil can represent a truck or an airplane to which the child applies appropriate actions. The toddler also begins to apply past experiences to new ones, although not always appropriately, indicating that the child is <u>thinking</u>, at least in some primitive sense. An interesting characteristic of play behavior is the often incredible repetitiveness of it; that is, the child repeats a set of actions over and over apparently in an effort to master the particular skills involved. (It might be noted that this behavioral

characteristic is not solely restricted to the toddler stage, but in fact can be observed at later age levels.) Despite the phenomenal growth in symbolic development, there remains a set of characteristics that reduce cognitive performance at this stage to relative immaturity. First, the toddlers thinking of abstractions are, in fact, relatively concrete or lacking in generality. In effect, the child has not formed recognizable concepts so that objects cannot be classified in terms of similarities. Secondly, the child's mental activities are largely egocentric; that is, his thinking is largely in terms of his own needs. Empathic behavior, for example, is unlikely because the child is unable to mentally place himself in the situation of another. This interesting aspect of the child's behavior is reflected in his question asking behavior, particularly about physical causality. Rather than reflecting a request for a causal (factual) explanation, the child is apparently much more concerned about his relationship to the particular event. For example, the toddler may accept the notion that there is snow on the ground because he wants to go skiing. It should be made explicitly clear that this characteristic of the toddlers' behavior is cognitive and not a personality characteristic in the usual sense of the term egocentricity. And, finally, the toddler is apt to distort reality to conform to his own desires.

Although this account of theoretical aspects of the toddlers' cognitive development is very general and lacking detail, it should be apparent that during this age period there is the emergency of symbolic behavior. Specifically, the child is moving away from

purely sensori-motor actions to increasingly greater reliance on symbolic or representations modes of behavior that rely less and less on actions. It is also clear that an important mode for expressing and for developing symbolic competency is through play and imitation -- modes which are particularly adaptive to the more formalized settings in which groups of toddlers are likely to be found. Theoretical considerations clearly indicate that toddler age children should be strongly encouraged to engage in symbolic play and that materials and instructional programs should be developed to foster such activities.

Again, the kinds of questions raised with respect to other components of the generalized model are completely relevant here. In addition, if we extend the toddler period through age four, it is quite possible that many children in a particular sample would be placed in preschools and day care programs. These aspects of their lives would also require observation and it would appear that similar observational dimensions would be equally relevant.

Before concluding this paper, I would be remiss if I did not at least briefly comment on certain design problems. It should be clear that the "naturalistic" work that derives from my fragmentary model does not permit clean univariate comparisons. Take the following example from a report I just read: mothers of Head Start children who participate more in the Head Start program, and community affairs, in general, have children who perform better, both intellectually and socially, in the program. Two conclusions were drawn from these studies: (1) Head Start stimulates greater

parental participation, and (2) this participation helps the children. This may be the case, but these data do not permit this conclusion. Quite possibly, Head Start parents who are active in the community were active before Head Start, are brighter and better educated, and had children whose entering levels were higher. If these variables had been partialled out of the outcome measures (child's achievement, parent participation), and the same results were observed, then there would be a basis for the conclusions. In other words, in designing long term comparative ecological studies it is absolutely crucial that adjustments be made on outcome measures as a function of entering measures in order that as pure an index of the SES related variables be obtained. There are statistical procedures which are readily available for this kind of analysis and must be used.

Conclusions

A very sketchy attempt has been made in this paper to lay out the various parameters required to determine what qualities of environments influence the social and cognitive development of children. An explicit assumption was made that SES as a gross variable is, for research purposes, useless and continued use of it as a blocking variable will not add new knowledge. As my sketchy model suggests, there is no global model available and the first step towards understanding social class will be the development of such a model. The second phase, data collection, will be expensive, difficult, and time consuming. But if we are

ever to go beyond meaningless rubrics and produce the information necessary for designing educational programs (in the broadest sense) this work must be achieved

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APPENDIX

Outline for PEM Study Adopted for Planning Purposes

(Detailed changes have been made by Task Groups at the discretion of group members.)

1000. PEM Aspects of Child Development

1100. Special Problems in Infancy and Early Childhood (birth to 5 years)

1101. Group care
   1. Effects of orphanage rearing, multiple mothering vs one-to-one mother-child (or surrogate mother) relations
   2. Related effects of environmental complexity

1102. Separation anxiety: fear of the strange

1103. Readiness
   1. General concept
   2. Special application to disadvantaged children

1104. Forced training ("pushing")
   1. In relation to "natural" intellectual limits
   2. In relation to readiness

1105. Sequential organization of learning
   1. In infancy
   2. In early childhood

1106. Parental involvement and influence on early development
   1. Effects of home environment, of implicit theories and practices of parents
   2. Manipulation of parental beliefs and practices, in enrichment programs

1107. Modes of learning and experience that affect early behavioral development
   1. Differential effects on anatomical maturation and behavioral development
   2. Correspondence between rates of anatomical and behavioral development
   3. Effects of environmental (experiential) enrichment and impoverishment, and cumulative effects with increasingly complex circumstances
   4. Hierarchical conceptions of intellectual development (Piaget)
   5. Development of learning sets and their implications for intellectual, motivational, and personality development; resistance of resultant behaviors to extinction
   6. Critical periods

1200. Child Socialization

1201. Conceptualization of the socialization process
   1. Socialization pressures
   2. Learning paradigms: e.g., dependency relations and adult control of "effects" (reinforcement), reference group formation

1202. Internalization of beliefs and values
   1. Conceptualization of attitude, belief, and value systems
   2. Identification processes
   3. Impulse control (self control)
   4. Effects of environmental resources

1203. Cognitive socialization
   1. Psycholinguistic structures, language development: effects on thought, beliefs, attitudes, interests; patterns of expression, values
   2. Uncertainty and information-seeking
   3. Development of expectancies; category accessibility; assimilation; effects on perception, cognition, action
   4. Symbolism, symbolic behavior

1300. Personality Development

1301. Developmental theories (Freud, Erikson, Piaget, Sears)

1302. Developmental sequences, stages
   1. Critical periods
   2. Fluid and crystallized patterns of intelligence (Cattell)

1303. Development of self-identity
   1. Self concept, ego theories, self theories
   2. Relations to social class, racial-ethnic factors, region, sex, family characteristics

1304. Effects of age, sex, culture, and other environmental factors

1305. Development of mechanisms of coping and adaptation

1400. Behavior Change

1401. Personality, learning

1402. Susceptibility to change of personality traits, attitudes, interests, beliefs, values

1403. Measurement of change

1404. Genetic, maturation, and learning factors in physical and psychological growth

2000. Personality

2100. Conceptual and Theoretical Approaches

2101. Criteria for a viable theory

2102. Development of unified, integrated theoretical formulations
   1. Cross-level comparisons and correlations
   2. Developmental histories of stable traits
   3. Relations among trait patterns at various developmental levels
   4. Relations of traits to perceptual responses in person perception and interpersonal interaction

2200. Cognitive Conceptions

2201. Cognitive style, complexity
2202. Balance theories
2203. Cybernetic formulations
    1. Computer simulation of personality
    2. Mathematical models

2300. Developmental Approaches (see 1300)

2400. Dynamic Approaches (see 1303, 4000)

2500. Morphologic Approaches

2600. Physiologic, Psychophysiological, and Biochemical Approaches (see 2102.1)

2700. Trait Structure, Multivariate Approach - Taxonomy of Trait-Explanatory Concepts of Stylistic and Temperament Aspects of Personality
2701. Methodological problems: definition of universes of behaviors for self-report, observation-rating, and objective test studies, cross-media matching of stable structures, design paradigms, including multi-modality designs and trait x treatment designs; construct validation of traits; effects of age, sex, sample, culture, and other environmental effects, and relations of these to resulting trait patterns; the range of roles and sets in relation to diversity of response patterns obtained (social desirability, acquiescence, and other specific sets), their similarities in terms of effects on self-description, and the relations of traits to moderator variables representing such sets
2702. Observational, rating methods: rater and "ratee" sources of effects in peer and "other" ratings, in observational trait assessment, and in interpersonal interaction; explicit concern with task, stimulus presentation, response format, socio-environmental setting, and demographic characteristics of participants; conceptual and empirical relationships among similar and related trait descriptors within observational-rating subdomain and in other subdomains (self-report)
2703. Self-report methods: item pools; format; item vs cluster factorization; measurement of and correction for response bias or distortion; development of a unified, consistent conceptual framework for concepts of personality style and temperament
2704. Objective test, misperceptive, indirect assessment, and development of fresh, new approaches to personality measurement and description

2800. Creativity
2801. Conceptualization of creativity; relations to intelligence, personality factors

2802. Characteristics of the creative person
2803. Analysis of the creative process
2804. Characteristics of the creative product
2805. Characteristics of the creative situation, short- and long-term; situational factors contributing to creative performance
2806. Measurement of creativity

3000. Emotions

3100. State Patterns: Physiological, Cognitive, Behavioral
3101. Arousal stimuli
3102. Response dimensions
3103. Uniqueness
3104. Learned-unlearned dimensions
3105. Affective learning; autonomic and physiological learning

3200. Relations to Traits, Roles

3300. Moderation of Expression by Learning
1. Culture patterns
2. Age, sex, group norms

3400. Drug Effects on Emotional Patterns

3500. Differentiation of States, Reflecting Situational, Organismic, and Stimulus Variations, from Traits, Represented as Long-Term Individual Dispositions

3600. Arousal States: Adrenergic Response, Stress

3700. Dysphoric States: Anxiety, Depression, Guilt, Shame, Remorse (see 4300)

3800. Duphoric States: Happiness, Elation, Joy, Hope, Confidence

4000. Motivation

4100. Conceptualization and Theory (human motivation)
4101. Homeostatic systems, physiological need
4102. Need-press system (Murray), subsystems (n Ach)
4103. Dynamic systems (Freud, Cattell)
4104. Cognitive and cybernetic approaches: motivation inherent in information-processing functions (Hunt), cognitive dissonance theory, incongruity, collative variables (Berlyne), balance theories, exchange theory
4105. Motivation inherent in individual performance, competence motivation (White)
4106. Trait systems and patterns (Guilford, Cattell)
4107. Values systems, moral character
4108. Conceptualization of interest, attitude, need, belief, value, ideal

4200. Process and Trait Formulations
4201. Relations and differences in conception and approach
4202. Process theories and formulations
  1. Balance theories
  2. Exchange theory
4203. Trait formulations: motives, values, character traits
  1. Methodology of measurement: Strong paradigm, Thurstone scales, Likert scales, Cattell's and Campbell's indirect approaches: self-report, objective, misperception, observation, rating, content analysis, unobtrusive measures
  2. Analytic approaches: factor analysis, multidimensional scaling, profile clustering
  3. Factored patterns of sentiments, attitudes, interests, beliefs, values
  4. Variations related to age, sex, sample, culture, and other environmental factors

4300. Frustration, Stress, and Anxiety
4301. Frustration theory and research evidence
4302. Conceptualization of stress
  1. Relation to frustration (Selye)
  2. Utility of stress concept in interpretation of behavior
  3. Relationships among physiological and psychological aspects
  4. Stress and coping, adaptation
4303. Adaptation-Level Theory (Helson) (see 5100)

4400. Conflict
4401. Conceptualization of conflict (Miller, Murphy, Cattell)
  1. Types of conflict: role, value, internal
  2. Approach and avoidance relations
4402. Conflict measurement and calculus
4403. Conflict in relation to interpretation and prediction of action

4500. Interests and Vocational Guidance
4501. Incremental value of interest measurement over ability and aptitude measures in predictions of various criteria on various populations (Thorndike, 10,000 Occupations; Clark, Minnesota study)

5000. Environmental Variables

5100. Conceptualization of Environmental Variables and Their Effects on Behavior; Human Ecology

5200. Methodologies for Encoding Environmental Factors

5300. Taxonomic Systems of Environmental Variables

5400. Normative Studies of Selected Behaviors in Relation to Defined Patterns of Environmental Setting: Sampling Problems in Relation to Populations, Behaviors, Macro- and Micro-Environmental Settings

6000. Interpersonal Behavior Processes

6100. Group Theory, Role Theory, Interpersonal Settings

6200. Interpersonal Perception, Attraction, Influence; Social Acuity, Empathy

7000. Variations in Psychological Processes

7100. Paradigms for such Research, Taking Account of Persons, Tasks, Environmental Settings, and Occasions (Cattell covariation chart, Campbell-Fiske model, longitudinal replication)

7200. Paradigmatic Studies of Selected Learning, Motivation, Perception, and Other Psychological Processes to Investigate Variations Attributable to Shifts in Subject, Task, Setting, and Occasion Dimensions

7201. Analyses to estimate magnitudes of variance components in standard dependent variables accounted for by trait, treatment, and trait by treatment sources and their specific constituents

7202. Analysis of total interaction parameter estimates into principal components or other dimensions in order to compare results by such methods with conventional R, P, Q analysis, both with single dependent variables and vectors (multiple dependent variables)